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Discussions

# *Interactive comment on* "Linkage of cave-ice changes to weather patterns inside and outside the cave Eisriesenwelt (Tennengebirge, Austria)" *by* W. Schöner et al.

**Anonymous Referee #2**

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The paper investigates determinants of ice growth and loss in alpine caves, an important problem for management of commercial caves and in protecting fragile ice formations from damage by visitors or broader climate changes.

The approach initially adopted is quite formal in presenting ice surface- boundary layer and energy budget for an ice mass balance. The presentation implies that a complete characterisation of fluxes and net melt/accumulation is both feasible and planned. Unfortunately, many of the formulae for terms in the energy budget are not accurately developed, or omitted (e.g. advective energy/mass transfer by water flow!). Some formulae might be justified if they were used to make rapid sensitivity analyses. For example a quick assessment of down welling longwave radiation (modified from eq 5) will show that it has negligible influence on melting. The absence of any references in this section implies that this section is either self-evident (in which case it is not needed) or original (which it is not). One is left concluding that the main objective of the section is to establish credibility by conspicuous wielding of equations.



The instrumentation has obviously worked well over quite a sustained period, though there is no commentary on the practical aspects of such successful deployment. Unfortunately, the instrumentation does not appear to be compatible with the preceding theory on energy budgeting. The boundary layer gradient approach developed is not pursued, nor would it be compatible with the non-equilibrium boundary layer described, so that virtually none of the budget terms outlined in theory can be estimated. The reader is left wondering why the elaborate boundary layer theory is needed to allow an essentially qualitative analysis of the data. A more appropriate approach for these problems is the advective penetration model advocated by Wigley and Brown (1976).

The measurements are interesting and well worth reporting. But the authors face a common problem in adequately presenting long high frequency time series; many of their graphs are ineffective in communicating the key attributes that are bring interpreted. The high frequency data are almost impossible to read. For example in Figure 7 the temperature data should be low pass filtered and resolved to temperature differences. These differences need to be carefully plotted again wind expressed as approximate volumetric fluxes (+ve in, -ve out). The ice survey elevation data make the net melt energy inscrutable. The change in thickness would give this and allow comparison to similarly lumped energy flux terms.

The ice data seem to suggest overwhelming year-to-year differences that demand immediate explanation, otherwise subsequent generalised analysis makes little sense. If tourist operations are a significant influence on air movement, then surely this needs substantial data and some attempt to segregate the data into door-open and door-closed sets. The discussion of cryogenic carbonates sees like an afterthought. If the ice stratigraphy is important it needs full and early disclosure and description. The real message in the ice description is that there is a mineralised (liquid) water source that is being frozen and partially sublimated. The discussion of this poorly described phenomenon does not seem to draw systematically on the physics. The conclusion that the "ice mass changes are in fairly good agreement with the energy fluxes" is not supported as there is no quantitative statement of these terms. The interesting diurnal temperature records (Figure 8) present substantial anomalies, but the authors fail to clearly highlight the paradox and do not provide any useful analysis. Any cave climate study lacking a vertical profile of the cave (i.e. providing the fundamental advectional setting) is unlikely to make much headway.

Overall, the paper's strength is in providing a sustained data set describing cave temperatures and wind. Yet the boundary layer (energy budget) theory is not appropriately developed for the setting, nor usefully applied to the data set. More diligent data processing as a foundation for a systematic qualitative characterisation of the primary processes and the likely signatures could be much more useful. The analysis of the data does not get beyond a qualitative interpretation of poorly presented information which is unfortunate.

Detailed comments

Section 1. So the purpose of the paper is.....? The introduction suggests that ice stratigraphy can be interpreted through inverse modelling which requires a robust and unique means of deriving ice thickness (and composition?) from climatologically pertinent variables. In retrospect, there is little support for this grand scheme.

Section 2 Methodological concepts and data: this section has no references implying either it is self-evident or original. Sources for the many equations and claims should be provided.

Equation 3. Meltwater advection appears to be significant (there is no meteorological precedent for rapid accumulation of ice nor freezing of mineralised water) and advec-



tion is not in the equation.

Eq 5. Why are air and rock radiative terms additive? This seems like a very naïve expression of down welling longwave radiation to a surface. Best bet is to measure it directly, especially in a complex geometry. The net longwave can be approximated from $\sigma\Delta$T4 which for any likely $\Delta$T can be shown to be ->0, so can be ignored . Selecting a suitable emissivity is not easy, so most people close their eyes and assume it is constant.

Equation 7 and following: the turbulent exchange term still needs to be determined. The expression "is well explained" is not clear nor justified quantitatively because the boundary layer is not unbounded (there is a roof and walls) and gradients are not homogeneous (edge effects and adiabatic effects may occur), a fundamental assumption in gradient energy budget methods. Qualitatively, the influence of temperature and vapour pressure gradients is adequately presented. It is difficult to believe that stability pervades the system throughout the year. Cold air over warm (0C) ice will be unstable; a particularly likely condition in winter near a lower entrance.

Equation 8. The ground heat flux is important as a substantive term. It can be modelled fairly accurately based on surface temperature measurements and assuming reasonable simple boundary and initial conditions. If it is to be excluded, it requires a quantitative demonstration of its insignificance in the overall balance (EQ2).

Equation 9 a should be a (subscript). Note "a" has been previously defined as a melt term, so should not be used ambiguously. g=is not defined

Equation 11 and ff. The expression is a bit sanguine (non critical). It is not really stratification that drives the chimney effect winds it is hydrostatic imbalance in coupled columns. Job one is defining the approximate column geometry. Taking the hydrostatic case and extending it into wind dynamics is a much more challenging problem. The subsequent exclusion of water vapour from consideration may be a mistake as it implies RH$\sim$0. RH$\sim$100% is a more reasonable assumption if it is to be considered constant.

P1715 line 20 ff. The discussion here confuses mechanical advection (external pressure patterns) and external-internal pressure fluctuations with density contrasts (Chimney effect winds).

Section 2 measurements. It is difficult to link the measurement regime with the formal theory previously outlined. Sometimes, there is a lack of clarity in the language used. More important, it is not clear how the gradient approach is being applied from apparently single, edge-influenced (i.e. not amenable to boundary layer representation) measurement of wind speed, ice and air temperature and (apparently unsatisfactory) relative humidity. A critical consideration of the energy balance equation shows that only M is fully characterised by the stake and distance measurements (though for some reason never quantified in the theory; $\Delta M=Lf(\Delta zi-\Delta V/A)$, perhaps). The radiation terms lack rock and representative air temperatures, the sensible and latent heat terms lack diffusivities and representative air temperature and relative humidity. In addition, the ice surface temperature is a very poor datum for gradient methods. There is no ground heat flux term modelled or measured. Does it matter? And advection by liquid transport is not included (and it is clearly important if incoherent). It is not clear how the discussion of pressure-temperature relations is applied.

None of the analysis uses energy flux density or water equivalent melt rate. It is largely an analysis of the form of the primary data series. I suggest that the formal theory presented is not being used critically in attaining a practical energy budget and complete monitoring programme. It would be more straightforward for the reader to use the theory less rigorously to provide a qualitative basis for interpretation of the measurements of ice growth and meteorological variables. The problem with the setting is that the energy budget is strongly influenced by air and water advection that is incompletely characterised. Wigley and Brown (1976) provide a much more salient discussion of chimney effect winds and penetration distance into a cave.

Results: The most obvious problem to address is that the winter of 2006-7 is apparently quite different to the winter of 2007-8 and 2008-9. Similarly the 2008 build up is



not replicated. The temperature analysis does not immediately explain these inconsistencies. Contrary to the claim (p1719 line 10-12), the wind in the cave has a strong correlation to outside winds, suggesting physical forcing rather than simple chimney effects.

The more detailed data (fig 7) can not be assessed by the reader (as described in 1720 17-19). To make the claim better substantiated, the data might be low pass filtered to an appropriate frequency, and internal-external temperature differences plotted. The wind speed should be expressed as velocity vectors (velocity x direction). A shorter clearer time period might be used, and a plot of wind velocity against temperature difference provided.

1720 line 29 No reference to figure 9. Figure 10: the diurnal cycle is actually not easy to resolve on these graphs. Do you mean figure 8?. Rather than claiming the diurnal variation is due to door openings, present data to support the claim. This control makes interpreting figure 7 very difficult. How is a threshold external temperature excluded from consideration? 1721 line 10...is adiabatic warming likely? See Wigley and Brown for discussion of penetration distances and flow reversals. Provide a reference for assuming 100% saturation (1721 15-21). Winter winds are warmed and are unlikely to be exactly at 100% RH and so induce sublimation loss. Summer cold air drainage can initially precipitate hoar frost. (See W&B for discussion)

1722 6-16. The data indicate the direction and magnitude of a vapour pressure gradient. More precision will not resolve the problem of determining sublimation/evaporation. The situation is very difficult to model or monitor. In effect, the fine-sounding theory (e.g. eq 7) is actually not really applicable.

1722 17ff. The argument seems to get derailed here by discussing previous work under results. I assume that “carbon” is actually meant to be “carbonate”. So the point is that sublimation is demonstrated which implies an upward vapour pressure gradient when the temperature is below zero Celsius. It should be possible to quantitatively identify periods of sublimation loss and gain and evaporation-condensation. Condensate and hoar water should not contain carbonate, so you have a testable hypothesis that the bulk water contributing to ice formation is groundwater. Your closing remarks seem incompatible with sublimation loss dominance required to produce carbonate cryobanding. (There is a fairly useful literature on t his phenomenon. see Karel Žák, Bogdan P. Onac and Aurel Perşoiu 2008 Cryogenic carbonates in cave environments: A review. Quaternary International Volume 187, Issue 1, 15 August 2008, Pages 84-96 Archives of Climate and Environmental Change in Karst )

1723 12-15 The longwave might become a net contributor, but what is the source temperature from an atmosphere? The cave wall temperature might give an approximation. See discussion of eq 5 above which can now be applied to discover that the resulting melt is ∼10EE-10 - 10EE11mm/day. In other words, applying the theory can usefully dispense this discussion. It is not clear how the "ice temperature" was measured. Encapsulated Hobo recorders are not suitable nor are probes because they do not provide "surface" temperature. A remote thermal infrared thermometer might be better, but difficult to calibrate adequately. Instead an ice temperature profile can be used to extrapolate to an estimated surface temperature with the added advantage that a suitably sensitive unit could indicate the presence of liquid water (depending on the mineral composition of the ice and water.)

1726 10 "The ice mass changes are in fairly good agreement with energy fluxes" There was never any systematic presentation nor analysis of this. Given the heterogeneity of the ice change and the limited climate data, it is not going to be easy to obtain a reasonable resolution. As a start, the net ice changes over each observation period can be converted to a melt energy value. Similar integrations can be made for vapour and temperature x windspeed to get a surrogate measure of sensible and latent heat fluxes. Segregate into warm and cold ice conditions. These can be compared to one another. The advection of water was not discussed nor measured and may prove to be larger than any of these terms unfortunately.



1727 18ff. The summer air temperature data are indeed interesting and deserve greater consideration. The near entrance temperatures are higher than the internal temperatures which is not possible using a single conduit penetration model. The implication is that there is a secondary flow system influencing temperatures. Vertical section cave maps may reveal this if exploration is complete including the roof. The other feature is that there may be a correlation with external air temperature lagged by one day. This is not impossible, but my first question would be on the logger clock synchronisation. (I say this having done it myself!)

Table 1: Luftfeuchte=relative humidity. The sensor model should be provided, not just the manufacturer. The table could be enhanced by adding the approximate precision. (Assuming calibration has taken care of accuracy adequately)

Figure 1. Not sure what the grey shades and lines indicate on the plan. Vertical profile of the cave is more important than the plan for meteorological interpretation.

Figure 4. Not sure what the two vertical scales refer to. Not clear how a max, min and average are computed. The step-character suggests a finite resolution, but this should disappear in averaging many such discrete values. The figure caption and label indicate that this is "change" (i.e. $\Delta z$ in the respective interval). . but the graph looks like it is actually zt-z0, the elevation relative to an arbitrary datum (time zero?). The rate of change is probably more pertinent to the energy budget approach $\Delta z/\Delta t$), Clarify. Dates are hard to read and different in the two graphs.

Figure 5. See figure 4. These are elevations not "changes" I think. It is not clear which axis refers to which line. The lower right hand axis seems to be a different scale.
Figure 6b. Wind speed is not really expected to correlate between outside and inside. Within the cave wind speed may correlate, but is contingent on cross sectional area. Discharge would be a more appropriate measure of advectional forcing of the energy budget.

Figure 7. Is difficult to decipher. I suggest making the time axis readable and simpler

(label each month which is about the readability in subsequent figures as well). The wind velocity and direction should be combined to show inward and outward velocity (the product of the two graphs) or flow (x respective area).

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Interactive comment on The Cryosphere Discuss., 4, 1709, 2010.